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EINSTEIN'S LAW OF GRAVITATION[1]

The by-laws of our society make it one of the duties of its president to deliver an address before its members. This fact renders it necessary for the president to select a subject; and this year the selection is to a certain degree forced by the public press. When a daily newspaper considers Einstein's work on gravitation a topic of sufficiently general interest to devote to it valuable space and cable funds, surely here is justification for my selection of this as the subject of my presidential address.

Einstein's original memoirs upon gravitation appeared in the years 1916 to 1918; and there are two excellent papers in English expounding and explaining his method, one by Professor deSitter, of Leyden, and one by Professor Eddington, of Cambridge. While Einstein's work may be known to many of you either in its original form or in one of the two papers mentioned, I fear that the attention of most of us was first directed seriously to the matter by the articles in the newspapers to which I have referred. I confess that I was one of those who had postponed any serious study of the subject, until its immense importance was borne in upon me by the results of the recent eclipse expedition. I have all the enthusiasm of the discoverer of a new land, and feel compelled to describe to you what I have learned.

Albert Einstein, although now a resident of Berlin and holder of a research professorship of the Kaiser Wilhelm Institute, is legally a Swiss. He is forty-five years old and was for some time a professor in the Zurich Technical School, and later in the University of Prague. He is a man of liberal tendencies, and apparently one whom any of

[1] Presidential address delivered at the St. Louis meeting of the Physical Society, December 30, 1919.

us would be glad to welcome for personal reasons in our international meetings of the future. He protested against the famous manifesto of the German professors in 1914 and was one of the eager supporters of the German Republic when it arose from the wreck of the Empire.

But, in presenting the subject of Einstein's study of the law of gravitation, I must begin many years ago. In the treatment of Maxwell's equations of the electromagnetic field, several investigators realized the importance of deducing the form of the equations when applied to a system moving with a uniform velocity. One object of such an investigation would be to determine such a set of transformation formulæ as would leave the mathematical form of the equations unaltered. The necessary relations between the new space-coordinates, those applying to the moving system, and the original set were of course obvious; and elementary methods led to the deduction of a new variable which should replace the time coordinate. This step was taken by Lorentz and also, I believe, by Larmor and by Voigt. The mathematical deductions and applications in the hands of these men were extremely beautiful, and are probably well known to you all.

Lorentz' paper on this subject appeared in the Proceedings of the Amsterdam Academy in 1904. In the following year there was published in the *Annalen der Physik* a paper by Einstein, written without any knowledge of the work of Lorentz, in which he arrived at the same transformation equations as did the latter, but with an entirely different and fundamentally new interpretation. Einstein called attention in his paper to the lack of definiteness in the concepts of time and space, as ordinarily stated and used. He analyzed clearly the definitions and postulates which were necessary before one could speak with exactness of a length or of an interval of time. He disposed forever of the propriety of speaking of the "true" length of a rod or of the "true" duration of time, showing, in fact, that the numerical values which we attach to lengths or intervals of time depend upon the definitions and postulates which we adopt. The words "absolute" space or time intervals are devoid of meaning. As an illustration of what is meant Einstein discussed two possible ways of measuring the length of a rod when it is moving in the direction of its own length with a uniform velocity, that is, after having adopted a scale of length, two ways of assigning a number to the length of the rod concerned. One method is to imagine the observer moving with the rod, applying along its length the measuring scale, and reading off the positions of the ends of the rod. Another method would be to have two observers at rest on the body with reference to which the rod has the uniform velocity, so stationed along the line of motion of the rod that as the rod moves past them they can note simultaneously on a stationary measuring scale the positions of the two ends of the rod. Einstein showed that, accepting two postulates which need no defense at this time, the two methods of measurements would lead to different numerical values, and, further, that the divergence of the two results would increase as the velocity of the rod was increased. In assigning a number, therefore, to the length of a moving rod, one must make a choice of the method to be used in measuring it. Obviously the preferable method is to agree that the observer shall move with the rod, carrying his measuring instrument with him. This disposes of the problem of measuring space relations. The observed fact that, if we measure the length of the rod on different days, or when the rod is lying in different positions, we always obtain the same value offers no information concerning the "real" length of the rod. It may have changed, or it may not. It must always be remembered that measurement of the length of a rod is simply a process of comparison between it and an arbitrary standard, *e. g.*, a meter-rod or yard-stick. In regard to the problem of assigning numbers to intervals of time, it must be borne in mind that, strictly speaking, we do not "measure" such intervals, *i. e.*, that we do not select a unit interval of time

and find how many times it is contained in the interval in question. (Similarly, we do not "measure" the pitch of a sound or the temperature of a room.) Our practical instruments for assigning numbers to time-intervals depend in the main upon our agreeing to believe that a pendulum swings in a perfectly uniform manner, each vibration taking the same time as the next one. Of course we can not *prove* that this is true, it is, strictly speaking, a definition of what we mean by equal intervals of time; and it is not a particularly good definition at that. Its limitations are sufficiently obvious. The best way to proceed is to consider the concept of uniform velocity, and then, using the idea of some entity having such a uniform velocity, to define equal intervals of time as such intervals as are required for the entity to traverse equal lengths. These last we have already defined. What is required in addition is to adopt some moving entity as giving our definition of uniform velocity. Considering our known universe it is self-evident that we should choose in our definition of uniform velocity the velocity of light, since this selection could be made by an observer anywhere in our universe. Having agreed then to illustrate by the words "uniform velocity" that of light, our definition of equal intervals of time is complete. This implies, of course, that there is no uncertainty on our part as to the fact that the velocity of light always has the same value at any one point in the universe to any observer, quite regardless of the source of light. In other words, the postulate that this is true underlies our definition. Following this method Einstein developed a system of measuring both space and time intervals. As a matter of fact his system is identically that which we use in daily life with reference to events here on the earth. He further showed that if a man were to measure the length of a rod, for instance, on the earth and then were able to carry the rod and his measuring apparatus to Mars, the sun, or to Arcturus he would obtain the same numerical value for the length in all places and at all times. This doesn't mean that any statement is implied as to whether the length of the rod has remained unchanged or not; such words do not have any meaning—remember that we can not speak of true length. It is thus clear that an observer living on the earth would have a definite system of units in terms of which to express space and time intervals, *i. e.*, he would have a definite system of space coordinates (x, y, z) and a definite time coordinate (t); and similarly an observer living on Mars would have his system of coordinates (x', y', z', t'). Provided that one observer has a definite uniform velocity with reference to the other, it is a comparatively simple matter to deduce the mathematical relations between the two sets of coordinates. When Einstein did this, he arrived at the same transformation formulæ as those used by Lorentz in his development of Maxwell's equations. The latter had shown that, using these formulæ, the form of the laws for all electromagnetic phenomena maintained the same form; so Einstein's method proves that using his system of measurement an observer, anywhere in the universe, would as the result of his own investigation of electromagnetic phenomena arrive at the same mathematical statement of them as any other observer, provided only that the relative velocity of the two observers was uniform.

Einstein discussed many other most important questions at this time; but it is not necessary to refer to them in connection with the present subject. So far as this is concerned, the next important step to note is that taken in the famous address of Minkowski, in 1908, on the subject of "Space and Time." It would be difficult to overstate the importance of the concepts advanced by Minkowski. They marked the begining of a new period in the philosophy of physics. I shall not attempt to explain his ideas in detail, but shall confine myself to a few general statements. His point of view and his line of development of the theme are absolutely different from those of Lorentz or of Einstein; but in the end he makes use of the same transformation formulæ. His great contribution consists in giving us a new geometrical picture of their

meaning. It is scarcely fair to call Minkowski's development a picture; for to us a picture can never have more than three dimensions, our senses limit us; while his picture calls for perception of four dimensions. It is this fact that renders any even semi-popular discussion of Minkowski's work so impossible. We can all see that for us to describe any event a knowledge of four coordinates is necessary, three for the space specification and one for the time. A complete picture could be given then by a point in four dimensions. All four coordinates are necessary: we never observe an event except at a certain time, and we never observe an instant of time except with reference to space. Discussing the laws of electromagnetic phenomena, Minkowski showed how in a space of four dimensions, by a suitable definition of axes, the mathematical transformation of Lorentz and Einstein could be described by a rotation of the set of axes. We are all accustomed to a rotation of our ordinary cartesian set of axes describing the position of a point. We ordinarily choose our axes at any location on the earth as follows: one vertical, one east and west, one north and south. So if we move from any one laboratory to another, we change our axes; they are always orthogonal, but in moving from place to place there is a rotation. Similarly, Minkowski showed that if we choose four orthogonal axes at any point on the earth, according to his method, to represent a space-time point using the method of measuring space and time intervals as outlined by Einstein; and, if an observer on Arcturus used a similar set of axes and the method of measurement which he naturally would, the set of axes of the latter could be obtained from those of the observer on the earth by a pure rotation (and naturally a transfer of the origin). This is a beautiful geometrical result. To complete my statement of the method, I must add that instead of using as his fourth axis one along which numerical values of time are laid off, Minkowski defined his fourth coordinate as the product of time and the imaginary constant, the square root of minus one. This introduction of imaginary quantities might be expected, possibly, to introduce difficulties; but, in reality, it is the very essence of the simplicity of the geometrical description just given of the rotation of the sets of axes. It thus appears that different observers situated at different points in the universe would each have their own set of axes, all different, yet all connected by the fact that any one can be rotated so as to coincide with any other. This means that there is no one direction in the four dimensional space that corresponds to time for all observers. Just as with reference to the earth there is no direction which can be called vertical for all observers living on the earth. In the sense of an *absolute* meaning the words " up and down," " before and after," " sooner or later," are entirely meaningless.

This concept of Minkowski's may be made clearer, perhaps, by the following process of thought. If we take a section through our three dimensional space, we have a plane, *i. e.*, a two-dimensional space. Similarly, if a section is made through a four-dimensional space, one of three dimensions is obtained. Thus, for an observer on the earth a definite section of Minkowski's four dimensional space will give us our ordinary three-dimensional one; so that this section will, as it were, break up Minkowski's space into our space and give us our ordinary time. Similarly, a different section would have to be used for the observer on Arcturus; but by a suitable selection he would get his own familiar three-dimensional space and his own time. Thus the space defined by Minkowski is completely isotropic in reference to measured lengths and times, there is absolutely no difference between any two directions in an absolute sense; for any particular observer, of course, a particular section will cause the space to fall apart so as to suit his habits of measurement; any section, however, taken at random will do the same thing for some observer somewhere. From another point of view, that of Lorentz and Einstein, it is obvious that, since this four dimensional space is isotropic, the expression of the laws of elec-

tromagnetic phenomena take identical mathematical forms when expressed by any observer.

The question of course must be raised as to what can be said in regard to phenomena which so far as we know do not have an electromagnetic origin. In particular what can be done with respect to gravitational phenomena? Before, however, showing how this problem was attacked by Einstein; and the fact that the subject of my address is Einstein's work on gravitation shows that ultimately I shall explain this, I must emphasize another feature of Minkowski's geometry. To describe the space-time characteristics of any event a point, defined by its four coordinates, is sufficient; so, if one observes the life-history of any entity, *e. g.*, a particle of matter, a light-wave, etc., he observes a sequence of points in the space-time continuum; that is, the life-history of any entity is described fully by a line in this space. Such a line was called by Minkowski a "world-line." Further, from a different point of view, all of our observations of nature are in reality observations of coincidences, *e. g.*, if one reads a thermometer, what he does is to note the coincidence of the end of the column of mercury with a certain scale division on the thermometer tube. In other words, thinking of the world-line of the end of the mercury column and the world-line of the scale division, what we have observed was the intersection or crossing of these lines. In a similar manner any observation may be analyzed; and remembering that light rays, a point on the retina of the eye, etc., all have their world lines, it will be recognized that it is a perfectly accurate statement to say that every observation is the perception of the intersection of world-lines. Further, since all we know of a world-line is the result of observations, it is evident that we do not know a world-line as a continuous series of points, but simply as a series of discontinuous points, each point being where the particular world-line in question is crossed by another world-line.

It is clear, moreover, that for the description of a world-line we are not limited to the particular set of four orthogonal axes adopted by Minkowski. We can choose any set of four-dimensional axes we wish. It is further evident that the mathematical expression for the coincidence of two points is absolutely independent of our selection of reference axes. If we change our axes, we will change the coordinates of both points simultaneously, so that the question of axes ceases to be of interest. But our so-called laws of nature are nothing but descriptions in mathematical language of our observations; we observe only coincidences; a sequence of coincidences when put in mathematical terms takes a form which is independent of the selection of reference axes; therefore the mathematical expression of our laws of nature, of every character, must be such that their form does not change if we make a transformation of axes. This is a simple but far-reaching deduction.

There is a geometrical method of picturing the effect of a change of axes of reference, *i. e.*, of a mathematical transformation. To a man in a railway coach the path of a drop of water does not appear vertical, *i. e.*, it is not parallel to the edge of the window; still less so does it appear vertical to a man performing manœvres in an airplane. This means that whereas with reference to axes fixed to the earth the path of the drop is vertical; with reference to other axes, the path is not. Or, stating the conclusion in general language, changing the axes of reference (or effecting a mathematical transformation) in general changes the shape of any line. If one imagines the line forming a part of the space, it is evident that if the space is deformed by compression or expansion the shape of the line is changed, and if sufficient care is taken it is clearly possible, by deforming the space, to make the line take any shape desired, or better stated, any shape specified by the previous change of axes. It is thus possible to picture a mathematical transformation as a deformation of space. Thus I can draw a line on a sheet of paper or of rubber and by bending and stretching the sheet, I can make the line assume a great variety of shapes; each of these new shapes is a picture of a suitable transformation.

Now, consider world-lines in our four dimensional space. The complete record of all our knowledge is a series of sequences of intersections of such lines. By analogy I can draw in ordinary space a great number of intersecting lines on a sheet of rubber; I can then bend and deform the sheet to please myself; by so doing I do not introduce any new intersections nor do I alter in the least the sequence of intersections. So in the space of our world-lines, the space may be deformed in any imaginable manner without introducing any new intersections or changing the sequence of the existing intersections. It is this sequence which gives us the mathematical expression of our so-called experimental laws; a deformation of our space is equivalent mathematically to a transformation of axes, consequently we see why it is that the form of our laws must be the same when referred to any and all sets of axes, that is, must remain unaltered by any mathematical transformation.

Now, at last we come to gravitation. We can not imagine any world-line simpler than that of a particle of matter left to itself; we shall therefore call it a "straight" line. Our experience is that two particles of matter attract one another. Expressed in terms of world-lines, this means that, if the world-lines of two isolated particles come near each other, the lines, instead of being straight, will be deflected or bent in towards each other. The world-line of any one particle is therefore deformed; and we have just seen that a deformation is the equivalent of a mathematical transformation. In other words, for any one particle it is possible to replace the effect of a gravitational field at any instant by a mathematical transformation of axes. The statement that this is always possible for any particle at any instant is Einstein's famous "Principle of Equivalence."

Let us rest for a moment, while I call attention to a most interesting coincidence, not to be thought of as an intersection of world-lines. It is said that Newton's thoughts were directed to the observation of gravitational phenomena by an apple falling on his head; from this striking event he passed by natural steps to a consideration of the universality of gravitation. Einstein in describing his mental process in the evolution of his law of gravitation says that his attention was called to a new point of view by discussing his experiences with a man whose fall from a high building he had just witnessed. The man fortunately suffered no serious injuries and assured Einstein that in the course of his fall he had not been conscious in the least of any pull downward on his body. In mathematical language, with reference to axes moving with the man the force of gravity had disappeared. This is a case where by the transfer of the axes from the earth itself to the man, the force of the gravitational field is annulled. The converse change of axes from the falling man to a point on the earth could be considered as introducing the force of gravity into the equations of motion. Another illustration of the introduction into our equations of a force by means of a change of axes is furnished by the ordinary treatment of a body in uniform rotation about an axis. For instance, in the case of a so-called conical pendulum, that is, the motion of a bob suspended from a fixed point by a string, which is so set in motion that the bob describes a horizontal circle and the string therefore describes a circular cone, if we transfer our axes from the earth and have them rotate around the vertical line through the fixed point with the same angular velocity as the bob, it is necessary to introduce into our equations of motion a fictitious "force" called the centrifugal force. No one ever thinks of this force other than as a mathematical quantity introduced into the equations for the sake of simplicity of treatment; no physical meaning is attached to it. Why should there be to any other so-called "force," which, like centrifugal force, is independent of the nature of the matter? Again, here on the earth our sensation of weight is interpreted mathematically by combining expressions for centrifugal force and gravity; we have no distinct sensation for either separately. Why then is there any difference in the essence of the two? Why not consider them both as brought into our equations by the agency of mathematical transformations? This is Einstein's point of view.

Granting, then, the principle of equivalence, we can so choose axes at any point at any instant that the gravitational field will disappear; these axes are therefore of what Eddington calls the "Galilean" type, the simplest possible. Consider, that is, an observer in a box, or compartment, which is falling with the acceleration of the gravitational field at that point. He would not be conscious of the field. If there were a projectile fired off in this compartment, the observer would describe its path as being straight. In this space the infinitesimal interval between two space-time points would then be given by the formula

$$ds^2 = dx^2_1 + dx^2_2 + dx^2_3 + dx^2_4,$$

where ds is the interval and x_1, x_2, x_3, x_4, are coordinates. If we make a mathematical transformation, *i. e.*, use another set of axes, this interval would obviously take the form

$$ds^2 = g_{11}dx^2_1 + g_{22}dx^2_2 + g_{33}dx^2_3 + g_{44}dx^2_4 \\ + 2g_{12}dx_1dx_2 + \text{etc.},$$

where x_1, x_2, x_3 and x_4 are now coordinates referring to the new axes. This relation involves ten coefficients, the coefficients defining the transformation.

But of course a certain dynamical value is also attached to the g's, because by the transfer of our axes from the Galilean type we have made a change which is equivalent to the introduction of a gravitational field; and the g's must specify the field. That is, these g's are the expressions of our experiences, and hence their values can not depend upon the use of any special axes; the values must be the same for all selections. In other words, whatever function of the coordinates any one g is for one set of axes, if other axes are chosen, this g must still be the same function of the new coordinates. There are ten g's defined by differential equations; so we have ten covariant equations. Einstein showed how these g's could be regarded as generalized potentials of the field. Our own experiments and observations upon gravitation have given us a certain knowledge concerning its potential; that is, we know a value for it which must be so near the truth that we can properly call it at least a first approximation. Or, stated differently, if Einstein succeeds in deducing the rigid value for the gravitational potential in any field, it must degenerate to the Newtonian value for the great majority of cases with which we have actual experience. Einstein's method, then, was to investigate the functions (or equations) which would satisfy the mathematical conditions just described. A transformation from the axes used by the observer in the following box may be made so as to introduce into the equations the gravitational field recognized by an observer on the earth near the box; but this, obviously, would not be the general gravitational field, because the field changes as one moves over the surface of the earth. A solution found, therefore, as just indicated, would not be the one sought for the general field; and another must be found which is less stringent than the former but reduces to it as a special case. He found himself at liberty to make a selection from among several possibilities, and for several reasons chose the simplest solution. He then tested this decision by seeing if his formulæ would degenerate to Newton's law for the limiting case of velocities small when compared with that of light, because this condition is satisfied in those cases to which Newton's law applies. His formulæ satisfied this test, and he therefore was able to announce a "law of gravitation," of which Newton's was a special form for a simple case.

To the ordinary scholar the difficulties surmounted by Einstein in his investigations appear stupendous. It is not improbable that the statement which he is alleged to have made to his editor, that only ten men in the world could understand his treatment of the subject, is true. I am fully prepared to believe it, and wish to add that I certainly am not one of the ten. But I can also say that, after a careful and serious study of his papers, I feel confident that there is nothing in them which I can not understand, given the time to become familiar with the special mathematical processes used. The more I work over Einstein's papers, the more impressed I am, not simply by his genius in viewing the problem, but also by his great technical skill.

Following the path outlined, Einstein, as

just said, arrived at certain mathematical laws for a gravitational field, laws which reduced to Newton's form in most cases where observations are possible, but which led to different conclusions in a few cases, knowledge concerning which we might obtain by careful observations. I shall mention a few deductions from Einstein's formulæ.

1. If a heavy particle is put at the center of a circle, and, if the length of the circumference and the length of the diameter are measured, it will be found that their ratio is not π (3.14159). In other words the geometrical properties of space in such a gravitational field are not those discussed by Euclid; the space is, then, non-Euclidean. There is no way by which this deduction can be verified, the difference between the predicted ratio and π is too minute for us to hope to make our measurements with sufficient exactness to determine the difference.

2. All the lines in the solar spectrum should with reference to lines obtained by terrestrial sources be displaced slightly towards longer wave-lengths. The amount of displacement predicted for lines in the blue end of the spectrum is about one hundredth of an Angstrom unit, a quantity well within experimental limits. Unfortunately, as far as the testing of this prediction is concerned, there are several physical causes which are also operating to cause displacement of the spectrum-lines; and so at present a decision can not be rendered as to the verification. St. John and other workers at the Mount Wilson Observatory have the question under investigation.

3. According to Newton's law an isolated planet in its motion around a central sun would describe, period after period, the same elliptical orbit; whereas Einstein's laws lead to the prediction that the successive orbits traversed would not be identically the same. Each revolution would start the planet off on an orbit very approximately elliptical, but with the major axis of the ellipse rotated slightly in the plane of the orbit. When calculations were made for the various planets in our solar system, it was found that the only one which was of interest from the standpoint of verification of Einstein's formulæ was Mercury. It has been known for a long time that there was actually such a change as just described in the orbit of Mercury, amounting to 574″ of arc per century; and it has been shown that of this a rotation of 532″ was due to the direct action of other planets, thus leaving an unexplained rotation of 42″ per century. Einstein's formulæ predicted a rotation of 43″, a striking agreement.

4. In accordance with Einstein's formulæ a ray of light passing close to a heavy piece of matter, the sun, for instance, should experience a sensible deflection in towards the sun. This might be expected from "general" considerations. A light ray is, of course, an illustration of energy in motion; energy and mass are generally considered to be identical in the sense that an amount of energy E has the mass E/c^2 where c is the velocity of light; and consequently a ray of light might fall within the province of gravitation and the amount of deflection to be expected could be calculated by the ordinary formula for gravitation. Another point of view is to consider again the observer inside the compartment falling with the acceleration of the gravitational field. To him the path of a projectile and a ray of light would both appear straight; so that, if the projectile had a velocity equal to that of light, it and the light wave would travel side by side. To an observer outside the compartment, *e. g.*, to one on the earth, both would then appear to have the same deflection owing to the sun. But how much would the path of the projectile be bent? What would be the shape of its parabola? One might apply Newton's law; but, according to Einstein's formulæ, Newton's law should be used only for small velocities. In the case of a ray passing close to the sun it was decided that according to Einstein's formula there should be a deflection of 1″.75 whereas Newton's law of gravitation predicted half this amount. Careful plans were made by various astronomers to investigate this question at the solar eclipse last May, and the result announced by Dyson, Eddington and Crommelin, the leaders of astronomy in England, was that there was a de-

flection of $1''.9$. Of course the detection of such a minute deflection was an extraordinarily difficult matter, so many corrections had to be applied to the original observations; but the names of the men who record the conclusions are such as to inspire confidence. Certainly any effect of refraction seems to be excluded.

It is thus seen that the formulæ deduced by Einstein have been confirmed in a variety of ways and in a most brilliant manner. In connection with these formulæ one question must arise in the minds of everyone: by what process, where in the course of the mathematical development, does the idea of mass reveal itself? It was not in the equations at the beginning and yet here it is at the end. How does it appear? As a matter of fact it is first seen as a constant of integration in the discussion of the problem of the gravitational field due to a single particle; and the identity of this constant with mass is proved when one compares Einstein's formulæ with Newton's law which is simply its degenerated form. This mass, though, is the mass of which we become aware through our experiences with weight; and Einstein proceeded to prove that this quantity which entered as a constant of integration in his ideally simple problem also obeyed the laws of conservation of mass and conservation of momentum when he investigated the problems of two and more particles. Therefore Einstein deduced from his study of gravitational fields the well-known properties of matter which form the basis of theoretical mechanics. A further logical consequence of Einstein's development is to show that energy has mass, a concept with which every one nowadays is familiar.

The description of Einstein's method which I have given so far is simply the story of one success after another; and it is certainly fair to ask if we have at last reached finality in our investigation of nature, if we have attained to truth. Are there no outstanding difficulties? Is there no possibility of error? Certainly, not until all the predictions made from Einstein's formulæ have been investigated can much be said; and further, it must be seen whether any other lines of argument will lead to the same conclusions. But without waiting for all this there is at least one difficulty which is apparent at this time. We have discussed the laws of nature as independent in their form of reference axes, a concept which appeals strongly to our philosophy; yet it is not at all clear, at first sight, that we can be justified in our belief. We can not imagine any way by which we can become conscious of the translation of the earth in space; but by means of gyroscopes we can learn a great deal about its rotation on its axis. We could locate the positions of its two poles, and by watching a Foucault pendulum or a gyroscope we can obtain a number which we interpret as the angular velocity of rotation of axes fixed in the earth; angular velocity with reference to what? Where is the fundamental set of axes? This is a real difficulty. It can be surmounted in several ways. Einstein himself has outlined a method which in the end amounts to assuming the existence on the confines of space of vast quantities of matter, a proposition which is not attractive. deSitter has suggested a peculiar quality of the space to which we refer our space-time coordinates. The consequences of this are most interesting, but no decision can as yet be made as to the justification of the hypothesis. In any case we can say that the difficulty raised is not one that destroys the real value of Einstein's work.

In conclusion I wish to emphasize the fact, which should be obvious, that Einstein has not attempted any explanation of gravitation; he has been occupied with the deduction of its laws. These laws, together with those of electromagnetic phenomena, comprise our store of knowledge. There is not the slightest indication of a mechanism, meaning by that a picture in terms of our senses. In fact what we have learned has been to realize that our desire to use such mechanisms is futile.

J. S. AMES

THE JOHNS HOPKINS UNIVERSITY

LEARNED SOCIETIES, OLD AND NEW[1]

IT would tax the younger men of science beyond the compass of their imagination, if

[1] President's address at the fourth meeting of the Annual Conference of Biological Chemists, held

for a moment they should stop other activities in order that they might weigh the magnitude of their indebtedness to the scientific societies of the past. It would reduce them below any level of humility if they compared the service of the contemporary societies with those of their ancestors, from whom they are separated by many centuries.

What a glorious record of devotion, sacrifice, and heroism is the history of the early days of the Accademie del Cimento of Italy, of the Royal Society of England, of the Académie des Sciences of France, of the Scientific Societies of Germany.

Somewhere remote in your memory, vaguely and hazily, perhaps, there still lingers a recollection that the bearers of the illustrious names of Copernicus, Gallileo, Toricelli, nay even of Newton, were viewed by their contemporaries with profound suspicion, as dangerous troublemakers; and if the vocabulary of the sixteenth and seventeenth centuries had been as luxuriant as is ours today, those illustrious men might have been disposed of as Bolsheviki.

In the days when those societies came to life, experimentation was a dangerous business. Scholasticism, philosophy, and all classes of organized society, nobility, gentry, clergy were hostile to experimental science. And in spite of these obstacles the result of the efforts of the great pioneers of the seventeenth and of the early eighteenth centuries were preserved and further developed, and made the foundation of our present civilization. In a great measure the success was attained through the activities of the learned societies of those days.

One is filled with astonishment and admiration reading about the great vision of the founders of those academies. They saw clearly all the needs of the new science and of the new times and they grouped together by joint effort to accomplish what they could not do individually. Indeed, so much were they permeated by their desire to serve science, rather than the individual scientist, that often the personalities of the investigators were completely submerged in that of the institution as a whole. In the Accademia del Cimento, as an instance, all the work was published anonymously in the name of the academy. This is perhaps the most sublime example of self-obliteration in the service of an ideal ever known in the history of science.

in affiliation with the American Biochemical Society, in the lecture room of the department of biochemistry in the medical school of the University of Cincinnati, December 30, 1919.

This oldest of all European societies more than any other emphasized the preeminence of experiment, of creation of instruments, establishment of standards of measurements, over theory and hypotheses. "Probando et Reprobando" was their motto. And indeed the academicians have discharged their task admirably. The number of instruments they constructed is endless, the scientific facts they discovered still stand among the foundations of our present sciences. And Poggendorf, referring to the Accademia del Cimento, says: "Few bodies have so well fulfilled their aims . . . ," and further, "we stand to-day on their shoulders."

The aims of the Accademia del Cimento were adopted by the younger European Society which later received its charter from Charles II. as the Royal Society of England.

This society furthered all the ambitions of its Italian forerunner and amplified on it by its program of social activities. As the Cimento, the members of this society were encouraged through cooperation to improve the tools of the scientists. Thus their members perfected the telescope, devised a spring for watches, improved the microscope. They were constructing laboratories, organizing collections, and by every means were improving the equipment and facilitating the task of the investigator. In a letter to Boyle, Hooke writes:

> We are now undertaking several good things, such as the collection of a repository, the setting up of a chemical laboratory, a mechanical operatory, an astronomical observatory, and an optic chamber.

The great effort made by the society to furnish the English workers with the in-

formation acquired outside of England is demonstrated by the creating of the office of a special secretary whose aim it was to maintain correspondence with the scientific men of other lands, to collect foreign publications, to translate them, etc.

In those days when bringing out a book was quite an enterprise the society often undertook the publication of the important works of its members and of other scientists. Indeed through the activity of the Royal Society the world became acquainted with the work of Newton. Writes Newton to Oldenburg, one of the secretaries of the society:

> At reading your letter I was surprised to see so much care taken about securing an invention to me of which I have hitherto had so little value. And therefore, since the Royal Society is pleased to think it worth patronizing, I must acknowledge it deserves much more of them for that than of me, who, had not the communication of it been desired, might have let it still remain in private as it hath already some years.

Indeed to such an extent was the society concerned with the interests of investigators that Secretary Oldenburg devised a way of securing rights of priority even in unfinished investigations.

The emphasis of the Royal Society on social and practical service is seen from the following lines taken from the writings of Sprot, one of the historians of the Royal Society.

> They have propounded the composing of a catalogue of all trades, works and manufactures, taking notice of all physical receipts or secrets, instruments, tools and engines. . . . They have recommended advancing the manufacture of tapestry, silk making. . . . They have compared soils and clays for making better bricks and tiles. . . . They started the propagation of potatoes and experiments with tobacco oil.

Indeed one could continue for hours if he made it his task to enumerate all the important functions undertaken by the Royal Society of England. The history of the French "Académie des Sciences" is only a repetition with variations of the histories of the two forerunners, and very much the same may be said of the early history of the German learned societies, though they came to life many decades later.

And now let us pass decades and centuries and let us make an attempt to write the current history of our own learned societies. What is their social function? What is their contribution to the end of facilitating the task of individual workers? What initiative do they take in introducing scientific methods in the practical activities of our social life?

I fail to find the data on which to write this current history. True, the high specialization of science of to-day makes modern presentations less comprehensive and less thrilling than in the times of Newton and of Leibnitz. True, all the activities of the old scientific societies have been appropriated by special institutions: the university, the technical institution, the research institution, the government bureaus, by the laboratories in the industries, and true it is that present societies can not resume the activities of the old academies. Should the societies of to-day then hibernate 362 or 363 days a year and awaken only for the remaining two or three days in order that the members may be bored by listening to communications which they comprehend not, nor are desirous to comprehend? No, hibernate they need not unless they choose to do so by preference.

The great emergency of the past war has demonstrated how capable of initiative, of achievement, of inventiveness the modern American scientist is, once his interest is aroused, when he is called to join hands with his fellow workers.

The old problems are gone, but new ones are coming up every day. Ours is a large country with great natural resources. It is customary to refer to them as endless. The word is a misnomer, an invention of those in whose interest it is to use the resources recklessly. Human energy is needed to exploit these resources; and human energy is not boundless. Who shall devise methods to preserve our natural resources from devastation? Why not a scientific body, and particularly one composed of biochemists? Nearly two years ago the American Chemical Society

initiated a campaign for the establishment of a research institute of chemotherapy. For the last year the propaganda has painlessly died. Why this lack of perseverance? I can see the need of another institute which would embrace the study of all the materials employed in the industries engaged in the manufacture of agricultural and natural products. True, the industries have undertaken a considerable share of this work, but industries work for the profit of to-day and not for the preservation of national wealth of the future.

Referring again to the biological chemist who interests us particularly, I see his need for better laboratories, of better methods, of better standards; I see the needs that have been pointed out by several members of this conference, and which are placed on the program for discussion, and of a great many more needs. Surely the biological chemist is not the most favored son of society, of the university, or of the medical school.

I am glad that Dr. Gies brought you all together[2] and gave you the opportunity to inaugurate a new type of society, the aim of which is to enhance the social usefulness of the biological chemist, on the one hand, and, on the other, to improve his facilities for work, whether his work be teaching or investigating. Will this new society live to record important service, or will it vegetate a pale, colorless existence? This will depend on the spirit in which you join it. The prospect for service is before you. Once more I wish to compliment Dr. Gies on his vision.

P. A. Levene

The Rockefeller Institute for Medical Research

2 An allusion to the fact that the conference was organized at Dr. Gies's suggestion.

A BUST OF THE LATE PROFESSOR E. D. COPE

A bust in plaster of the late Edward Drinker Cope, who, at the time of his death in Philadelphia, on April 12, 1897, was professor of zoology and comparative anatomy in the University of Pennsylvania, has been purchased by the subscriptions of some twenty-seven of his former colleagues, associates and students and presented to the zoological laboratory of the university.

This bust is the work of Mr. Eugene Castello, of Philadelphia, and is the one represented in half tone in the number of *The American Naturalist* for May, 1897. Mr. Castello writes:

I had been engaged on portrait busts, of Dr. Matthew Woods, president of the Browning Society, and of Dr. William Mountain, author of "Saint Cecilia." The study of individual character in these portraits, followed by the production of a number of heads of racial types: American Indians, Russian moujiks, Arabs and Frenchmen, directed my attention to the very unusual features of Professor Cope's head. That he was quite aware of the interesting subject he was for a sculptor was soon evident, for he humorously described himself as "gimber-jawed," that is, he meant that the lower jaw was slightly undershot, having much the form of a skate runner extending from ear to chin.

In reference to the circumstances connected with the modelling of the bust, now the property of the university, I consulted a diary that I kept at that time and find that he gave me six sittings for it, beginning October 22, 1896, and the last one on January 6, 1897. At the final sitting he expressed himself as satisfied that I had succeeded in obtaining a good likeness. After Professor Cope passed away, his friend, Dr. Persifor Frazer, saw the bust and invited me to place it in the hall of the American Philosophical Society, May 7, 1897, where it remained for some time. Later it was again exposed there on the occasion of the Cope Memorial meeting [November 12, 1897], where it received favorable criticism from Professor Osborn of the American Museum of Natural History, Dr. Minis Hays and others. . . . Dr. Nolan, of the Academy of Natural Sciences, of this city, also has taken occasion to express his appreciation.

The work of constructive modelling of the head was aided to a considerable extent by the sitter himself, who seemed to be familiar with the anatomical points that differentiated it from any others and which attracted my attention when I met him for the first time. Artists delight in individual character, such as was evident in his head, and upon my expression of interest Professor Cope consented to give me some sittings, although suffering at the time with an incurable

malady. He collapsed on one occasion during a sitting and I was obliged to administer stimulants to revive him. He was a very patient sitter, although I knew he was suffering from disease, and had never before given a sitting to a sculptor.

I think the university is to be congratulated on obtaining possession of the work and I can assure you and the other subscribers that nothing could be more pleasing to me. It is an exact duplicate of the head even in measurement, every feature being transferred and reproduced in the clay by means of calipers, such as are used by sculptors, so that the work has a sort of scientific value as a human document. I used calipers with points especially protected with little cork balls. This seemed to amuse Professor Cope and yet he showed considerable fear that I might do some damage to his features with the instrument. The plaster bust was made from the clay by myself in a matrix of plaster which was destroyed in the process known to sculptors as the "waste mould process."

As far as known, this bust of Professor Cope is the only one in existence modelled from life, although a death-mask was taken and is preserved in the University Museum. Although he never saw the present zoological laboratory of the University of Pennsylvania, it seems fitting that this building, which houses his osteological collection and many of his books, should also be enriched by this bust.

Philip P. Calvert

The University of Pennsylvania

SCIENTIFIC EVENTS

THE HENRY PHIPPS INSTITUTE

The Henry Phipps Institute for the study and prevention of tuberculosis, a part of the University of Pennsylvania, is engaged in a campaign to raise $3,000,000 to enable it to continue its work. Dr. Charles J. Hatfield is executive director; Dr. H. R. M. Landis, director of the clinical and sociological departments, and Dr. Paul A. Lewis, director of the pathological department. The text of the institute's appeal is in part as follows:

Whereas, The support which has been so generously contributed during the past 16 years by Mr. Henry Phipps can no longer be extended;

Whereas, The board of trustees of the University of Pennsylvania see no prospect of being able to support the work of the Henry Phipps Institute from the funds at present available;

Whereas, It is deemed important that the work of the Henry Phipps Institute be continued upon an even larger scale:

The directors of the departments of the Henry Phipps Institute announce a campaign to raise a Foundation Fund of $3,000,000.

It is confidently expected that America will rally to the support of this enterprise which has already accomplished so much for the betterment of humanity in so difficult a field of endeavor.

The Henry Phipps Institute was the first organization brought into existence for the express purpose of eradicating tuberculosis through intensive and scientific research.

The institute was conceived when Dr. Lawrence F. Flick, about to start a tuberculosis clinic with a total backing of $1,000, met Mr. Henry Phipps by appointment and discussed the venture with him. Mr. Phipps at once offered to underwrite a much more extensive enterprise aimed at the extermination of tuberculosis.

On February 1, 1903, the institute began work in an old remodeled building equipped with 52 beds, a small laboratory and facilities for operating a large dispensary.

During the ten years that followed, its work was so successful that Mr. Phipps not only agreed to continue his support over another stipulated period of time, but also supplied funds for the purchase of land and the erection of the splendid property in which the institute is now housed.

In order that the standing of the institute might be assured and the integrity of the enterprise guaranteed, it was on July 1, 1910, placed in charge of the trustees of the University of Pennsylvania, with the contractual understanding that Mr. Phipps would be responsible for its support over a stipulated period of time.

The new building erected at Mr. Phipps' expense provided adequate facilities for every branch of medical and sociological research bearing upon the problem of tuberculosis.

The period for which Mr. Henry Phipps had agreed by contract to support the work of the institute came to an end in May, 1919. Because of ill health Mr. Phipps is not able to continue his interest and support. Other means of maintenance must be found or the institute must close. In this event one of man's strongest defenses in the battle against tuberculosis will be abandoned.

THE AWARD OF THE BOYLE MEDAL

The presentation of the Boyle Medal to H. H. Dixon on January 23, 1917, by Lord Rathdonnell is now a matter of somewhat ancient history to his colleagues of the Royal Dublin Society. Due to delay in transmission of periodicals, however, the account of the presentation and the bibliography of Dr. Dixon's more than three score contributions to science have only just reached America in printed form.[1] Because of the widespread interest in Dixon's work on the rise of water in trees, the writer is hastening at this late hour to do honor to a brilliant career and a gentleman of scientific vision.

The tension theory of the ascent of sap in trees was published in 1894 in collaboration with Dr. John Joly. The latter, also, is favorably known in America as a physical geologist and mineralogist and a graceful writer of essays on scientific topics, ranging all the way from the "Birth-Time of the World" to "Skating" and "Pleochroic Halos." He also visited the United States as a member of the British Education Commission two years ago. Many of Dr. Dixon's earlier researches were undertaken with Dr. Joly. Dr. Dixon's principal scientific labors may be classed under three main heads: Cytology and genetics, the path of the transpiration current, and cryoscopy and thermo-electric methods.

Contributions to cytology include fertilization of *Pinus sylvestris* and some significant work on reduction division and mitosis which aided about a decade later in the rediscovery of Mendel's law. However, transpiration soon began to be Dixon's chief topic of experiment and research and his results will doubtless remain one of the great contributions to botanical science. During the interval between 1894 and 1914 investigations concerning the resistance experienced by the transpiration stream and theories to account rationally for the upward movement of water were developed. Most of the methods employed in these researches were devised by Dr. Dixon and only a few were in collaboration with students. It is, then almost entirely due to his genius and patient effort that the epochal discoveries come into being. His records of this work are contained in the monograph "Transpiration and the Ascent of Sap," published about 1914. Previously he had been invited to contribute to *Progressus Rei Botanicæ* on the same subject. The third line of investigation has been largely in collaboration with Dr. W. R. G. Atkins. Osmotic pressure changes and cryoscopic and conductivity measurements on saps have been particularly dealt with. These researches are still continuing and have been amplified recently by new attacks on the many problems of photosynthesis, especially the increase of sucrose rather than the hexoses following insolation. There is no doubt but that much valuable information will result from this field of investigation.

The closing sentences of the biographical note (*loc. cit.*) seem to indicate that Professor Dixon has been accomplishing this magnificent amount of experimental work at the same time that he was teaching "large classes" of medical students. The more honor to him. One can not help feeling, however, the stupidity of university organization which permitted his time to be occupied during the best years of his life in work which was relatively unproductive for the science of botany. If such an inspired worker can not impress the governing board of the school with the importance of fundamental research, the outlook for most of us is indeed dark.

A. E. Waller
The Ohio State University

[1] Award of the Boyle Medal to Professor Henry Horatio Dixon, Sc.D., F.R.S., *Sci. Proc. Roy. Dublin Soc.*, 15: 179–184. Anon.

IN HONOR OF WILLIAM H. WELCH

On April 8 Dr. Welch reaches his seventieth birthday. Such an occasion ought not to pass without some new expression of affection and admiration on the part of the medical profession of America to one who has long stood as its leader. To many of his friends it has seemed that an expression worthy the master would be the preservation in suitable form of the chief contributions from his pen.

Dr. Welch's writings are scattered through a great variety of publications and are more or less inaccessible. It has accordingly been decided to bring together and to publish in three volumes his papers and addresses which strikingly reveal the great part he has played in the development of medical science and medical education.

In order that the project may be assured it has been decided to invite his friends and former pupils to unite in making possible the publication of his work.

The volumes will be issued by the Johns Hopkins Press under the editorial supervision of the undersigned committee. The set of three volumes, bound in linen, is offered to the subscribers at $16.50, which is less than the estimated cost. Each copy will be numbered, and assigned in the order of subscription. The edition will be restricted to the number subscribed.

Committee: John J. Abel, Lewellys F. Barker, Frank Billings, Walter C. Burket, William T. Councilman, Harvey Cushing, John M. T. Finney, Simon Flexner, William S. Halsted, William H. Howell, John Howland, Henry M. Hurd, Henry Barton Jacobs, William W. Keen, Howard A. Kelly, William G. MacCallum, William J. Mayo, Ralph B. Seem, Winford H. Smith, William S. Thayer, J. Whitridge Williams, Hugh H. Young.

SCIENTIFIC NOTES AND NEWS

SIR AUCKLAND GEDDES, who was formerly professor of anatomy in McGill University, and is now a member of the British cabinet as president of the board of trade, has been named as British ambassador to the United States.

DR. W. S. HALSTED, of the Johns Hopkins University, has been elected to honorary foreign membership in the Royal Academy of Medicine of Belgium.

THE following are the officers of the Association of American Geographers for the year 1920: *President,* Herbert E. Gregory; *Vice-presidents,* Harlan H. Barrows and Charles F. Brooks; *Treasurer,* George B. Roorback; *Councilors,* Walter S. Tower, Eliot Blackwelder and Ray H. Whitbeck; *Secretary and Editor,* Richard E. Dodge.

MAJOR H. E. WIMPERIS has been transferred from the office of the British Crown Agents for the Colonies to the Air Ministry, to take up the position of head of the air navigation research section.

MR. ALFRED SMETHAM, chemist to the Royal Lancashire Agricultural Society, has been elected president of the British Society of Public Analysts in succession to Dr. Samuel Rideal.

DR. LÉON BERNARD, professor of hygiene in the faculty of medicine, Paris, a well-known writer on tuberculosis, has been elected a member of the Academy of Medicine. Dr. Lesbre, of Lyons, and Dr. Lignières, of Buenos Aires, have been elected correspondents.

THE Christian Fenger fellowship for 1920 has been awarded to Dr. Harry Culver, of the University of Illinois Medical School, Chicago. He will continue his studies on Infections of the Kidney.

DR. ALBERT ERNEST JENKS, professor of anthropology and director of the four-year Americanization training course at the University of Minnesota, has been made president of the newly organized National Council of Americanization Workers.

JOHN WAGNER, JR., civil engineer, eldest son of Samuel Tobias Wagner, chief engineer of the Philadelphia and Reading Railway Co., has been elected a member of the board of trustees of the Wagner Free Institute of Science, to fill the vacancy caused by the death of Joseph Willcox.

DR. NATHANIEL L. BRITTON, director of the New York Botanical Garden, is engaged in botanical work in Trinidad.

DR. J. PERCY MOORE, professor of zoology in the University of Pennsylvania, has been given leave of absence for one year to study abroad.

PROFESSOR EMILIO ODDONE, an Italian seismologist, arrived recently in New York from

Naples on his way to Mexico to study recent earthquakes there for his government.

Frederic H. Lahee, formerly professor of geology at the Massachusetts Institute of Technology, and since the latter part of 1918 associate geologist for the Sun Oil Co., of Dallas, Tex., will take charge of the geological department of the Twin State Oil Co., at Tulsa, Okla., while still maintaining his connection with the Sun Co.

Mr. Rollin C. Dean, who for the last eight years has represented the Bausch and Lomb Optical Co. among the universities and colleges of the east, will become connected with The Rockefeller Foundation.

Dr. Mary J. Erickson has arrived at the University of Iowa to take charge of the research work in the state board of health under the recent appropriation from the federal government for investigation in the field of venereal diseases.

Professor C. E. Seashore, of the psychology department of the State University of Iowa, lectured on the "Psychology of Musical Talent" at the University of Kansas on March 1.

Dr. Christine Ladd-Franklin lectured recently before the Research Club of the Harvard Medical School on the theory of color sensation.

Professor Edward J. Moore, of the department of physics of the University of Buffalo, spoke before the Buffalo Society of Natural Sciences on February 3, on "The Einstein Gravitation Theory."

Professor Douglas W. Johnson, of Columbia University, addressed the faculty and students of Mount Holyoke College on February 18, on "The Work of the Geographer and the Geologist in the War."

Dr. W. H. R. Rivers, of the University of Cambridge, will lecture on "Ethnology: its Aims and Needs" at Columbia University on the evening of March 15. It will be a general meeting of the New York Academy of Sciences arranged by the Section of Anthropology and Psychology and the American Ethnological Society.

Herbert Ralph Werner, assistant professor of zoology in the Iowa State College, died on February 14, at the age of thirty-one years, of pneumonia following influenza.

Dr. Charles Gordon Hewitt, Dominion entomologist and consulting zoologist, died at Ottawa on March 1. He had resided in Canada since 1909, having been born in Scotland in 1886.

Sir Thomas Anderson Stuart, professor of physiology and dean of the faculty of medicine in the University of Sydney, died on April 3. He was born in Scotland in 1856.

The U. S. Civil Service Commission announces an examination for assistant fuel engineer. A vacancy in the Bureau of Mines, Department of Interior, at Pittsburgh, Pa., at $4,200 a year, will be filled from this examination.

House tariff measures fixing duties on optical glass, laboratory apparatus, surgical instruments and glass and porcelain articles for laboratory use have been ordered favorably reported by the Senate Finance Committee.

Through the courtesy of the American Geographical Society the spring meeting of the Association of American Geographers will be reinaugurated this year. The meeting will be held in New York City at the American Geographical Society's hall, April 16 and 17, 1920. All interested are most cordially invited to attend.

The Iowa Academy of Science will hold its thirty-fourth annual meeting at the University of Iowa on April 30 and May 1, under the presidency of Professor T. C. Stephens, of Morningside College. It is expected that fully one hundred papers on scientific subjects will be presented.

The next annual meeting of the British Medical Association will be held in the University of Cambridge at the end of June under the presidency of Sir Clifford Allbutt. It was intended to hold the 1915 meeting at Cambridge under his presidency, but the war intervened and he has remained president of the association.

YALE UNIVERSITY has recently received from Bayard Dominick, of the class of 1894, Yale College, gifts amounting to $40,000 for scientific exploration in the Southern Pacific Ocean. Professor Herbert E. Gregory, of Yale, is the active head of the expedition, and the funds will be disbursed by the Bishop Museum of Honolulu. It is expected that the work of the expedition will extend over a period of two years and that it will be carried on by a group of distinguished men of science. Professor Gregory has been granted leave of absence for the balance of the year by Yale and is now in Honolulu.

A NEW museum has been opened at Yellowstone Park, Wyoming, for the preservation and exhibition of natural history specimens of the region.

THE fortieth annual report of the United States Geological Survey, made public, compares the present scope of the work with that of the work done during the first year of this organization. The growth of the survey is suggested by a comparison of the appropriations for the present year, which comprise items amounting to $1,437,745, with the total appropriation of $106,000 for the first year, 1879–80. During the 40 years the number of employees has been increased from 39 to 967.

THE arrangements for the amalgamation of the four existing British meteorological services are practically completed, and it is expected that at an early date the reorganization, which will combine the Meteorological Office with the weather services of the Air Ministry, the Navy, and the Royal Engineers, will be effected under the Department of the Controller-General of Civil Aviation, and will be directed by Sir Napier-Shaw, the present Director of the Meteorological Office at South Kensington. The headquarters of the amalgamated services will be at the Air Ministry, Canada House, Kingsway. It is understood that the forecasting department and other departments of the Meteorological Office will be transferred from South Kensington to the Air Ministry, while the statistical department and the library will remain at the present office in Exhibition Road. The British Rainfall Association, which was founded in 1860, and which has been a very successful private enterprise, will come under the director of the Meteorological Office, but it is expected that its special work will continue to be carried on at Camden-square. The combined services will be in close touch with all the colonial and foreign observatories and the Air Minister will assume Parliamentary responsibility for the new combined department.

THE Advisory Committee at the American Chemical Society, on recommendation of Editor E. J. Crane, has passed the following vote:

That *Chemical Abstracts* be empowered to loan to members in good standing of the American Chemical Society, copies of current publications upon request; that each such request must be accompanied by twenty-five (25) cents for each issue requested to cover cost of packing, mailing and correspondence, and must further be accompanied by an undertaking on the part of the requesting member to replace such issue or issues, should they not be returned to *Chemical Abstracts* in good order, less reasonable wear and tear; *Chemical Abstracts* to notify the loaning member of receipt in good or bad order, as the case may be, of the loaned issue and then to close the transaction accordingly.

THE Oberlin College Research Committee, affiliated with the National Research Council, met recently for dinner and the transaction of business at the Faculty Club. The present committee consists of men engaged in experimental scientific work, but a recommendation was adopted to include those from the mathematics department. Discussion centered around possible methods of stimulating and financing research in those departments which care to do such work, and also the development of research spirit as a definite college policy. It was definitely expressed as the opinion of those present that a college of the standing of Oberlin must abandon the policy that teaching is the sole business of the faculty members, and that productive work must be given the prominence it merits.

THE United States Committee on the Ramsay Memorial Fund has transmitted

£3,500 which it has collected; £263 have been sent direct by contributors; approximately £100 yet remain in the hands of the treasurer, Mr. W. J. Matheson. Professor Baskerville, the chairman, hopes that the total American contribution which is £3,863, may be raised to £4,000, and that the American subscriptions may then be closed. The total fund now amounts to £51,274. Professor H. Kamerlingh Onnes reports contributions of £1,571 given or promised by donors in Holland.

Robert W. Lawson writes to *Nature* from the Physics Laboratory, the University of Sheffield, quoting a letter of Professor Einstein as follows: "Zwei junge Physiker in Bonn haben nun die Rot-Verschiebung der Spektral-Linien bei der Sonne so gut wie sicher nachgewiesen und die Gründe des bisherigen Misslingens aufgeklärt."

Mr. Theodore W. Robinson, of Chicago, has given $500 to be used in purchasing museum material for the Oriental Institute of the University of Chicago; and a donor whose name is withheld gives $25,000 for the same purposes. These funds will be used by Professor James Henry Breasted, who is now in Egypt on his way to Mesopotamia.

The National Research Council has received a gift from the Southern Pine Association of $10,000 to pay for the incidental expenses of a coordinated scientific study by a number of investigators of the re-growth of trees or cut-over forest lands with the aim of determining the best forestry methods for obtaining the highest productivity. The investigation will be conducted under the advice of the Research Council's special committee on forestry and will not duplicate any present government or other undertakings along similar lines.

On the invitation of the council of the senate of the University of Cambridge, the chancellor, the vice-chancellor, Mr. Rawlinson, Professor Sir Joseph Larmor, Professor Sir J. J. Thomson (master of Trinity), Dr. Hobson, and Professor Sir Ernest Rutherford, have consented to serve as representatives of the university on a joint committee of the Royal Society and university for the purpose of taking steps to secure an appropriate memorial to the late Lord Rayleigh.

UNIVERSITY AND EDUCATIONAL NEWS

Professor William H. Walker, chairman of the administrative committee of the Massachusetts Institute of Technology, since the death of President Maclaurin, has resigned to devote his time to the division of industrial cooperation and research. The new chairman is Professor H. P. Talbot, chairman of the faculty. Professor E. B. Wilson, of the physics department has been appointed a member of the committee, on which is also Professor Edward Miller, of the department of mechanical engineering. Professor Walker is succeeded as head of the course of chemical engineering by Professor Warren K. Lewis. As has been already noted here, Professor Arthur A. Noyes, head of the research department, has handed in his resignation as of January 1, to go to the California Institute of Technology.

After thirteen years of service as professor of medicine and ten years as dean of the Yale School of Medicine, Dr. George Blumer has resigned to resume consultation practise, but he will not wholly sever his connection with the school and the hospital.

Dr. Arthur B. Lamb has been promoted to a professorship of chemistry at Harvard University.

Dr. Adolph Knopf, of the U. S. Geological Survey, has been appointed lecturer in geology in Yale University for the second term of the present academic year. He has in charge the undergraduate and graduate courses in petrology formerly taught by the late Professor Pirsson. Additional appointments in the geological department are those of Dr. Carl O. Dunbar (B.A. Kansas 1913, Ph.D. Yale 1917) as assistant professor of historical geology, and Mr. Chester R. Longwell (B.A. Missouri 1915, M.A. 1916) as assistant professor of geology.

THE trustees of Cooper Union, New York City, have authorized the organization of a four-year day course in industrial chemistry to be started in September of the present year. This course will aim to train men as analysts, research chemists, foremen and superintendents in manufacturing plants, and sales agents. Mr. Maximilian Toch, has been appointed adjunct professor of industrial chemistry.

DR. H. E. ROAF has been appointed to the university chair of physiology tenable at the London Hospital Medical College, and Professor T. Swale Vincent to the university chair of physiology tenable at the Middlesex Hospital Medical School.

DISCUSSION AND CORRESPONDENCE

AN ODD PROBLEM IN MECHANICS

TO THE EDITOR OF SCIENCE: The following statements are intended to throw light on the questions raised by Dr. Hering in his letter entitled "An odd problem in mechanics" in SCIENCE for January 9, 1920.

The statements in the second paragraph of the letter are correct: a body travelling eastward on the ground along the equator will exert less pressure on the ground than one at rest relative to the earth's surface, and still less pressure than a body travelling westward. The correctness of this statement was verified experimentally in connection with observations to determine the intensity of gravity at sea by determinations of the boiling point compared with readings of the mercury barometer. In the spring of 1909 the Russian government placed a war ship at the disposal of Professor Hecker, who was engaged in this work, and tests were made in the Black Sea by comparing the gravity obtained when the ship was running east with gravity at the same point when the ship was running west. The correction in question is of the order of 0.100 dyne for a vessel of fair speed, and the reality of the expected effect and the necessity of applying a correction for it were, of course, verified. It should be mentioned that the rolling, pitching and lifting of the ship, which occur on all courses, were such that the total effect of the ship's motion did not necessarily reverse in sign when the ship's course was reversed.

In the third paragraph it is assumed that the "gyroscopic tendency (of a rotating horizontal flywheel) to get into the vertical plane has been counteracted and may be neglected." But the forces Dr. Hering has been describing in this paragraph are exactly the gyroscopic forces themselves that tend to make the axis of the flywheel parallel to the earth's axis. At the equator, since the celestial pole is in horizon, the plane of the flywheel would tend to become vertical. If the gyroscopic tendency is counteracted, there is, of course, no shifting of the axis of rotation.

In the cases supposed in the fourth paragraph, there are gyroscopic forces arising from the earth's rotation that Dr. Hering has not considered. When the plane of rotation is north and south, that side of the disk which is descending will tend to move eastward, and the side that is ascending will tend to move westward, thus tending to turn the plane of the disk out of the meridian into the prime vertical, so that its axis shall be parallel to the axis of the earth. The apparatus will therefore not be dynamically balanced as Dr. Hering states. At the equator there is no twisting effect due to the horizontal motion of the particles on the edge of the disk, for this effect varies as the sine of the latitude. At the equator, when the plane of the disk is east and west, its axis is parallel to the earth's axis, and the apparatus is dynamically balanced.

The nature of the general question raised may be stated in a few words as follows. For a body at rest on the earth, it is sufficient to consider only the attraction of the earth and the centrifugal force due to the earth's rotation. For a body in motion relative to the earth, there are additional apparent forces to be considered, the so-called gyroscopic forces, or compound centrifugal forces. These apparent forces arise from the fact that our axes of reference are not fixed in direction in space, but are rotating. These forces are all proportional to the product of the earth's angular velocity of rotation by a component velocity

along one of the moving axes; furthermore, all components of relative velocity, northward, eastward, or upward (and their opposites) give rise to these forces. Dr. Hering's argument from the varying centrifugal force due to the east and west motion of a particle brings to light the gyroscopic forces due to the east-and-west components of velocity, but it does not tell the whole story. Vertical components, and horizontal components in the meridian must also be allowed for.

There is nothing very new in the results stated above. Problems of moving axes and the effect of the earth's rotation are treated in much detail in advanced treatises like Routh's "Rigid Dynamics." The equations of motion for these cases can be conveniently ground out by Lagrange's method, but it is always interesting and instructive to obtain each term in the result directly, and to examine its geometrical and mechanical meaning.

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QUOTATIONS

FEDERATIONS OF BRAIN WORKERS

In the discussion on the better adjustment of the relations between employers and employed which have occupied so much space in the public press during the last year or so attention has been almost exclusively directed to the relations of industrial employers and manual workers. The interests of other classes of persons whose work is essential to industry have been almost ignored, although the Labor Party has declared its willingness to accept recruits from among brain workers. At the industrial conference summoned by the Prime Minister last April employers' associations and trade unions considered a proposal for a joint industrial council, and the Society of Technical Engineers at this conference moved an instruction to the council, when it should come into existence, to consider the position of unions composed exclusively of members of technical, management, and administrative grades, and to determine how such unions should be represented on the council. The industrial council has not yet come into existence, but meanwhile the Labor Research Department has been making inquiries into the position of professional classes in relation to the labor movement, and at a meeting in London on February 7, a National Federation of Professional, Technical, Administrative, and Supervisory Workers was formed. The bodies represented at this conference included the Civil Servants Union, the Association of Local Government Board Officers, the National Union of Clerks, the National Federation of Law Clerks, the National Union of Journalists, representatives of scientific, technical, engineering, and chemical workers, together with the Actors' Association and the National Orchestral Association. A representative of the Labor Research Department said that it was not proposed that the Federation should affiliate with the Labor Party or the Trade Union Congress. Among the professions invited to join the new Federation medicine and the law are not included. It appears, however, that for some months past certain technical and scientific professional workers have been taking steps to form themselves into a confederation, and that representatives of these bodies and several others, after full discussion, have prepared a memorandum proposing that the various societies concerned should be formed into an industrial group, a financial group, a group for the public services, and a group for the other professions. Each group would form a federation, and the four would be combined into a confederation for which draft rules are being prepared. The General Secretary of the Society of Technical Engineers last week published a long letter on the subject in *The Times,* in the course of which he observed that the assumption that a salaried official must ally himself either with the employers or with the work-people ought not to be accepted without further investigation. The position of medicine and the law are similar to each other and differ fundamentally from that of the intellectual workers represented by such bodies as the Society of Technical Engineers. The medical profession

will be disposed to watch with sympathetic interest the movement for a federation of scientific and technical workers; but until their plans are more fully known it will be premature to say that medicine should have any direct concern.—*British Medical Journal.*

SCIENTIFIC BOOKS

The Productivity of Invertebrate Fish Food on the Bottom of Oneida Lake, with Special Reference to Mollusks. By FRANK COLLINS BAKER. Technical Publication No. 9, New York State College of Forestry at Syracuse University, Syracuse, N. Y. 1918. Pp. 233, Figs. 44.

This valuable contribution to the general subject of limnology is based upon a numerical study of the bottom fauna of a portion of Oneida Lake, New York, which was made during the month of July, 1916. Lower South Bay and two smaller areas, all at the southwestern corner of the lake, were covered in the survey; they constitute an area of 1,164 acres, or a little less than two square miles out of a total lake surface of about 80 square miles. The maximum depth of the water in the area under consideration is about 19 feet as compared with a maximum of 55 feet for the entire lake.

In the area covered by this survey the greatest development of plant and animal life was found in the zone extending from the shoreline out to the six-foot contour line. Numerically, about 88 per cent. of the invertebrate animals were obtained in this area. The second zone lay between the six-foot and the twelve-foot contour lines and the population of this belt was very much smaller than in the first zone. A still further decline in the density of the population was noted between the twelve-foot and the eighteen-foot contour lines, which constituted the third zone.

Various types of bottom were found in the area studied, ranging from boulders to clay and mud. Of those represented, the sand bottom was richest in animal life while the boulder bottom was poorest.

A classification of the animals on the basis of their feeding habits showed that herbivorous and detritus feeders greatly predominated over the carnivorous forms; the latter, in fact, constituted only 0.29 per cent. of the total population. Of the various groups of animals represented, the mollusks yielded a much larger number of individuals than any other group; they even exceeded in numbers all of the associated animals combined.

CHANCEY JUDAY

MADISON, WISCONSIN

SPECIAL ARTICLES

THE ANTISCORBUTIC PROPERTY OF DEHYDRATED MEAT

THE present conception of a perfect diet demands that the intake contain adequate proteins, sufficient fats, carbohydrates, inorganic salts, bulk, and the three vitamines designated as water-soluble B, fat-soluble A, and antiscorbutic. For some time we have used to produce experimental scurvy in guinea-pigs a combination which meets all of these requirements except that of the antiscorbutic vitamine. A mixture of soy bean flour, whole milk, dried yeast, paper pulp, sodium chloride and calcium lactate is dried down into a cake.[1] This is fed as the basal ration supplemented with a definite amount of the product whose antiscorbutic potency it is desired to determine. By this procedure we have demonstrated that dried cabbage,[1] dehydrated tomatoes[2] and desiccated orange juice[3] retain some of their original content of antiscrobutic vitamine.

The indications are that each foodstuff ought to be studied individually. Meat being one of the most staple articles of our dietaries it has therefore seemed highly important to determine if it retains any antiscorbutic potency after drying.

Stefansson[4] states that "the strongest anti-

[1] Givens, M. H., and Cohen, B., *J. Biol. Chem.*, 1918, 36, 127.

[2] Givens, M. H., and McClugage, H. B., *J. Biol. Chem.*, 1918, 37, 253.

[3] Givens, M. H., and McClugage, H. B., *Am. J. Dis. Chil.*, 1919, 18, 30.

[4] Stefansson, V., *J. A. M. A.*, 1918, 71, 1715.

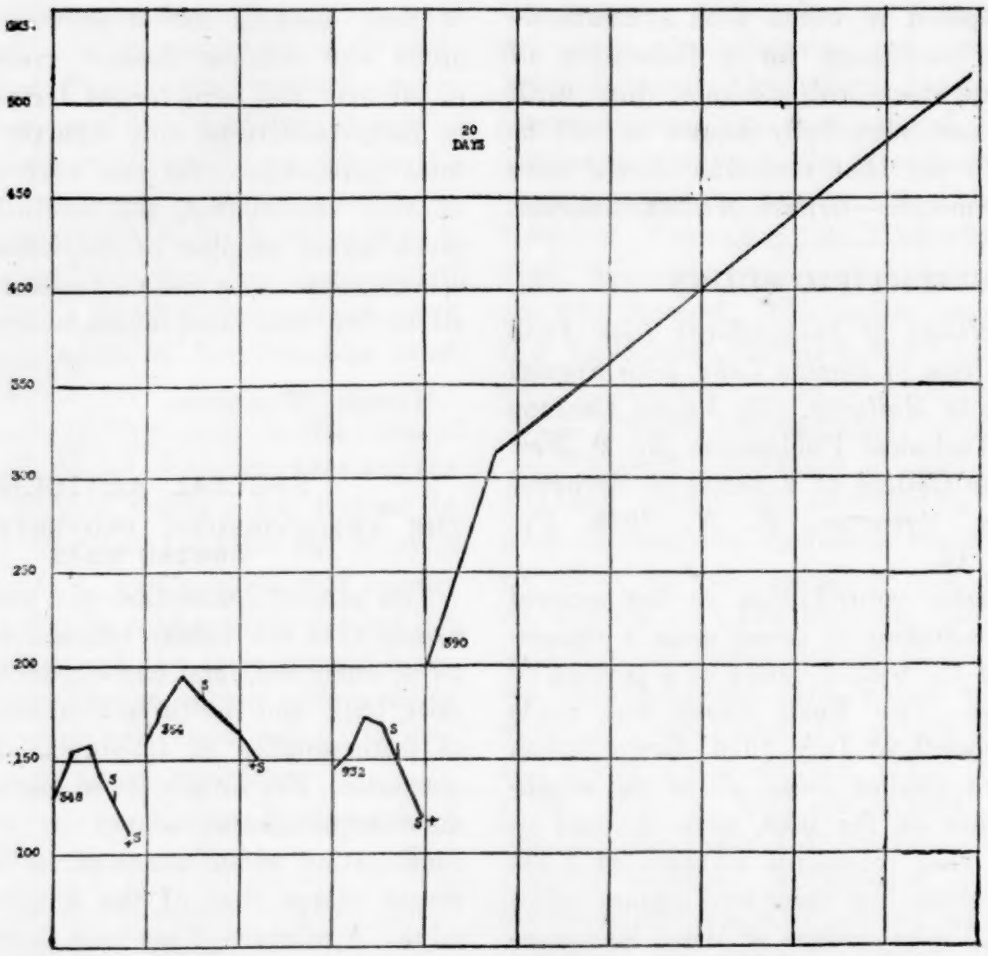

FIG. 1. Growth curves of guinea-pigs on different diets.

Animal No. 932 is representative of a number of guinea-pigs receiving the soy cake diet without any supplement. Clinical signs of scurvy were first noted on the 14th day; death from scurvy occurred on the 19th day. Animal No. 390 receiving soy cake plus 30 gms. of raw cabbage daily never showed any signs of scurvy up to the 120th day, when it was transferred to other experiments. These two groups serve as controls to show that the soy cake diet *alone* will not prevent scurvy but that it is satisfactory if supplemented with a good antiscorbutic agent as, in this case, raw cabbage.

Animal No. 348 is typical of a group on the soy diet plus a daily supplement of dehydrated beef. Animal No. 354 is one of a number of guinea-pigs receiving the soy cake diet plus an allotment of desiccated beef cooked for 15 minutes at 100° C. In these two groups the development of scurvy has not been prevented nor death from the disease delayed.

S signifies first appearance of scurvy.

S + death from scurvy.

scorbutic qualities reside in certain fresh foods and diminish or disappear with storage by any of the common methods of preservation—canning, pickling, drying, etc. Meat and fish slightly or well advanced in the process of ordinary putrefaction seems to be as good an antiscorbutic as fresh flesh or nearly so." Notwithstanding the above statements Stefansson used some dried meat on one of his polar expeditions. However, circumstances were such as not to permit a long usage of the dried products and therefore no direct evidence is available in his cases as to the antiscorbutic value of this material.

Chick, Hume and Skelton[5] found that 10 c.c. of raw fresh beef juice daily did not prevent scurvy in a guinea-pig on a diet of oats and bran ad lib.

Pitz[6] has offered experiments to show that

[5] Chick, H., Hume, E. M., and Skelton, R. F., *Biochm. J.*, 1918, 12, 131.

[6] Pitz, W., *J. Biol. Chem.*, 1918, 36, 439.

5 per cent. of dried meat does not delay the onset of scurvy but does greatly prolong the life of the animals, while 10 per cent. of this meat delays the onset of the disease and greatly prolongs the life of the animals. He also thinks that calcium and chloride cause delay in the development of scurvy.

Dutcher[7] and his associates claim that raw lean beef does not possess antiscorbutic properties. They think the favorable influence from dried meat claimed by Pitz is in reality due to the fact that the animals in those experiments were consuming milk ad lib.

The dried meat used in our experiments was lean beef freed of fat and dehydrated in vacuo at a temperature never higher than 65° C. for a period of twelve hours.[8] The meat was then air dried for several days, during which time it gave up a little more moisture. This dried product was ground to a powder and offered as such to the animals. The guinea-pigs did not care for the food in this form and the only satisfactory consumption obtained was through intimately blending the meat with the soy cake food by grinding the two together. By this manipulation an average consumption of fifty per cent. or better of the 3 gm. of meat offered daily, was obtained from all animals. The actual daily amount of dried meat eaten was about 1.5 gm. per guinea-pig; representing approximately 15 per cent. of the total solids ingested.

The dried meat was fed uncooked and cooked for fifteen minutes at 100° C. In neither case was there any protection against the onset of scurvy nor was death therefrom delayed. A graphic presentation of the above results is given in the chart by a curve of growth of a typical animal from each group.

The findings in these animal experiments are in accord with those of Chick, Hume and Skelton and of Dutcher and associates on the value of raw meat juice and raw meat and a watery extract of raw meat. The results support Stefansson's contention, in so fas as meat is concerned, that foodstuffs preserved by desiccation are deficient in their antiscorbutic property.

The meat used by Pitz in his experiments was dried over steam coils. Our results are in direct opposition to his. The explanation of this is undoubtedly due, as Dutcher believes, to the amount of milk consumed by the guinea-pigs in Pitz's experiments. His results in all likelihood would have been the same as ours had the intake of milk been controlled quantitatively.

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[7] Dutcher, R. A., Pierson, E. M., and Biester, A., SCI., N. S., 1918, 50, 184.

[8] Our thanks are due Dr. K. Geo. Falk, of the Harriman Laboratories, Roosevelt Hospital, New York City, for kindly supplying us with the meat used in these experiments.

THE AMERICAN METEOROLOGICAL SOCIETY

THE American Meteorological Society was organized in St. Louis, on December 29, 1919 (*cf.* preliminary announcements, SCIENCE, August 22, 1919, pp. 180–181, and December 12, 1919, pp. 546–547). Following the organization, the Council of the American Association for the Advancement of Science granted affiliation. The officers elected for 1920 are: R. DeC. Ward, president; W. J. Humphreys, vice-president; Robert E. Horton, treasurer, and Charles F. Brooks, secretary. Fifteen councilors representing the various phases of theoretical and applied meteorology were also elected. They are: Lieutenant Colonel W. R. Blair, Meteorological Service, Signal Corps, Washington; E. H. Bowie, Weather Bureau, Washington, D. C.; Professor H. J. Cox, Weather Bureau, Chicago, Ill.; A. W. Douglas, Simmons Hardware Co., St. Louis, Mo.; Professor Ellsworth Huntington, Yale University, New Haven, Conn.; Lieutenant C. N. Keyser, Aerology Division, U. S. Navy, Washington, D. C.; Professor C. F. Marvin, Weather Bureau, Washington, D. C.; Major General C. T. Menoher, Air Service, Washington, D. C.; J. C. Millas, Meteorological Service, Habana, Cuba; James H. Scarr, Weather Bureau, New York, N. Y.; Professor J. Warren Smith, Weather Bureau, Washington, D. C.; Sir F. Stupart, Meteorological Office, Toronto, Canada; Professor C. F. Talman, Weather Bureau, Washington, D. C.; Dr. F. L. West, Utah Agricultural College, Logan, Utah; Professor W. M. Wilson, Cornell University, and Weather Bureau, Ithaca, N. Y. Eleven committees

were formed to carry out the objects of the society. These with their chairman are: Research, C. F. Marvin; Public Information, C. F. Talman; Metrological Instruction, W. M. Wilson; Membership, C. F. Brooks; Physiological Meteorology, Ellsworth Huntington; Agricultural Meteorology, J. Warren Smith; Hydrological Meteorology, R. E. Horton; Business Meteorology, A. W. Douglas; Commercial Meteorology, H. J. Cox; Marine Meteorology, J. H. Scarr; Aeronautical Meteorology, Major General C. T. Menoher.

On December 30 and 31, in St. Louis, and on January 3, in New York, 29 papers were presented in five sessions. There was one joint session with the American Physical Society, and one with the Association of American Geographers and National Council of Geography Teachers. Since brief abstracts of each paper are published in the January issue of the *Bulletin* of the American Meteorological Society, and more extensive abstracts, excerpts, or the papers in full, covering all but nine, in the December *Monthly Weather Review*, only the titles and authors will be given here:

Progress of American meteorology in 1919: C. F. Brooks.

Some meteorological paradoxes: W. J. Humphreys.

How the American Meteorological Society can serve geography teachers: C. F. Brooks.

Use of laws in teaching climatology: S. S. Visher.

Motion pictures of weather maps: a report of progress: J. Warren Smith.

The work of the Weather Bureau in the West Indies: O. L. Fassig.

Aims and achievements of the Blue Hill Observatory: A. McAdie.

Aerological work in the U. S. Navy: C. N. Keyser.

Plans for establishing a network of meteorological stations in Palestine: P. W. Etkes.

Determination of the normal temperature by means of the equation of the seasonal temperature variation and of a modified thermograph record: F. L. West, N. E. Edlefsen and S. P. Ewing.

The roaring of the mountain: W. J. Humphreys.

Some applications of radio-telegraphy to meteorology: J. C. Jensen.

Sunshine in the United States: R. DeC. Ward.

Cloudiness in the United States: R. DeC. Ward.

Weather conditions in the orchard regions of the North Carolina mountain slopes: H. J. Cox.

The effect of a "lid" on the temperature and transparency of the lower air: J. W. Redway.

Preliminary steps in making free-air pressure and wind charts: C. L. Meisinger.

The prevailing winds of the north Pacific coast: A. E. Caswell.

Evaporative capacity: R. E. Horton.

A device for measuring maximum and minimum temperatures of reservoir surfaces: R. E. Horton.

Clouds and their significance: C. F. Brooks.

Difficulties in the theory of rain formation: W. J. Humphreys.

Cultivation does not increase rainfall: J. Warren Smith.

Predicting minimum temperatures: J. Warren Smith.

Seasonal distribution of maximum floods in the United States: A. J. Henry.

Weather and business: A. W. Douglas.

Explanation of peculiarities in flying in the wind: J. G. Coffin.

Determination of meteorological corrections on the ranges of guns: W. Noll.

Evidence of climatic effect in the annual rings of trees: A. E. Douglass.

On January 21, the society was incorporated in the District of Columbia. The membership of the society, elected up to the end of January, was 586.

The next meeting of the American Meteorological Society will be held in Washington, D. C., probably, Thursday, April 22, immediately preceding that of the American Physical Society, on Friday and Saturday, April 23 and 24. Plans are being made for meetings with the Pacific Section of the American Association for the Advancement of Science next summer and with the American Association for the Advancement of Science in Chicago next December.

Charles F. Brooks,
Secretary

Weather Bureau,
Washington, D. C.

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